**SENIOR REVIEW AND FUNDING RECOMMENDATION**
***Salton Sea Financial Assistance Program – FY 2012-2013***

- ✓ **Applicant:** University of California Berkeley, Department of Plant & Microbial Biology; Dr. Norman Terry, Principal Investigator
- ✓ **Project Title**: Field Test of a Novel Wetland Treatment System to Provide Clean Water for the Salton Sea Species Conservation Habitat
- ✓ **County:** Imperial
- ✓ **Grant Request:** $1,065,993
- ✓ **Total Project Cost:** $1,401,343

**Project Description:** This project application proposes the construction of a Treatment Wetland test system composed of a sedimentation basin, eight long-length treatment cells, and eight short-length treatment cells. The applicants are partnering with the Imperial Irrigation District (IID) for the construction phase of the project and thereafter will share operation and maintenance costs with the IID. The test system will be utilized to conduct four experiments: (1) Determining the relationship between water residence times of 3, 6, 9, and 12 days and magnitudes of selenium and nutrient removal; (2) Evaluation of efficacy of algal pre-treatment component at different algal densities (with the rates of selenium volatilization estimated via mass balance calculations); (3) Comparative performance trials for candidate species of algae, including matched laboratory direct measures of Se volatilization as a check against the mass balance calculation method employed for the field trials; and (4) An attempt to confirm a hypothesized correlation between the build-up of soil organic matter (SOM) over time and temporally increasing selenium and nutrient treatment performance (across a 3-year time horizon). In addition to the sampling of water, sediment, cattails, and algae associated with the four experiments listed above, a biological sampling program to include biofilms, macroinvertebrates, fish, amphibians, and avian excreta is proposed to facilitate ecological risk assessment via speciation of selenium in these samples using high-energy synchrotron-based x-ray absorption spectroscopy (XAS) conducted at the Stanford Synchrotron. This project will build upon specific bench scale research already completed, and more generally upon the principal investigator's 15-20 years of prior research in this field of study. The goal, after three years, would be to provide a cost effective and environmentally safe wetland treatment system that could provide high quality water for fish and wildlife conservation habitats.

**Summary**

| Criteria | Score | Factor | Total |
|---|---|---|---|
| 1. Consistency with Program goals and objectives | 3 | 7 | 21 |
| 2. Applicant qualifications | 3 | 3 | 9 |
| 3. Project Readiness | 3 | 3 | 9 |
| 4. Feasibility | 0 | 7 | 0 |
| | | **Total Score** | 39 |

**Consistency with Program goals and objectives:** Generally this proposal addresses a critical issue for Salton Sea restoration, namely, how to reduce selenium in waters being used for restoration. If successful, this project will lead to improving living conditions of fish and wildlife using the Salton Sea. The key here are the

words "If successful..." And while the track record for this research team indicates that they do work that is very valuable and productive academically, there is almost no chance that the work would lead to any operationally useful end product and therefore would not really be consistent with Program goals and objectives. For example, at Chevron Marsh, the Se content of water running through the wetland was measured as declining by 89% (Hansen et al. 1998) before and by 84% several years after Chevron received the results of the UC Berkeley research (CH2M HILL final monitoring report circa 2002). Clearly, the Se removal aspect of the marsh was not changed by any of the knowledge of Chevron gained. Management Marsh did turn out to be a success story, but only from a wildlife management perspective, not from a Se management perspective. More importantly, the applicants' earlier multiple years of work on agricultural drainage water at TLDD was an operational failure that TLDD managers described as producing a system that was "too expensive" and "too complicated" to implement. Albeit, the system being proposed for testing now is somewhat different from systems previously field tested for TLDD, but is also even more complicated than what was already too complicated to be cost effective, and the crucial algal pre-treatment component for the new system was presented with an estimate of performance efficiency for Se volatilization (up to 50%) that can be accepted only on pure faith. No data or literature citations were provided to support the stated performance assessment. Finally, the potential for creating a toxic hazard for wildlife was not adequately addressed. This is immensely important. If such a hazard were to be created, the net outcome of this proposal would not only fail to be consistent with Program goals and objectives, but would actually work against them.

**Applicant qualifications:** The applicants have been conducting related and directly relevant research on selenium uptake and volatilization by plants for some 15-20 years. They are extremely well qualified to address those components of what is being proposed. However, any proposal to use a wetland system to "treat" selenium must match the Se treatment component with an equally rigorous fish and wildlife risk assessment component. The applicants seem to be aware of this to some extent. As they state in their proposal, "***Depending on the chemical species of Se building up in the sediments, there is a potential risk of Se ecotoxicity***." Of course, the potential for ecotoxicity depends on far more than simply the speciation of Se in sediments; and the biological sampling program included in the proposal is very poorly designed for evaluating potential ecotoxicity; which in turn was viewed by the review team (with full consensus) as flowing from the fact that the research team simply does not include a member with sufficient background and expertise to design, conduct, and interpret a rigorous fish and wildlife risk assessment component.

**Project Readiness:** Bench-scale pilot research previously completed and the proposed working partnership with the Imperial Irrigation District positions this proposal very favorably with regard to readiness. However, there are also uncertainties regarding whether the final permitting and construction work still required for this proposal might not introduce the potential for significant delays regarding a fully operational start date.

**Feasibility:** The goal of this research is to provide a cost-effective source of high quality (low Se) water for wetland creation and maintenance. One of the most important facts that must be established to assess the degree of success being achieved is what the Se bioaccumulation potential would be for the product water intended for provision to fish and wildlife habitats. However, there is no component of the proposal that could establish this fact. This is a fatal deficiency for the feasibility of the project, i.e., no factual measure of

ultimate success or failure. To address this deficiency, the applicants should review Amweg et al. (2003). The bioaccumulation bioassay trials for product water described and conducted by Amweg et al. (2003) are precisely what should be added to this proposal to address the environmental safety of the product water. As Amweg et al. so clearly demonstrated, it is not enough to merely establish that the total concentration of Se in product water has been reduced. It has not been sufficiently documented that the algal pre-treatment cells are at all likely to achieve meaningful amounts of Se volatilization. Selenium volatilization is described in this proposal as a pivotal step in successful treatment. The statement (see *Proposed Research* section)–***Algae have a considerable propensity to take up and volatilize Se directly; our research has shown that as much as 50% of the Se may be removed in this way***–is not referenced nor is there discussion based on current literature of the degree of uncertainty in this measurement. Mean or overall Se volatilization from laboratory or field studies of soil-bacterial-algal-plant systems shown in the literature is quite low (0.5%- 9.4%) (Gao et al., 2003; Lin and Terry, 2003; Vale Braga, 2011; Lin et al., 2000; Azaizeh et al., 2003). The applicants cited a value of 20-30% Se volatilization at Chevron Marsh (Hansen et al. 1998), but that seems odd considering that elsewhere they have explained why the Chevron results are not relevant to treatment of agricultural drainage water (Terry 2002). For example, the Chevron measurements were made during only the summer when volatilization rates are seasonally highest; the Chevron Marsh is a selenite Se system, not a selenate Se system; and the Chevron Marsh is a low sulfate system, not a high sulfate system. Consequently additional documentation would be necessary to support a reasonable expectation of feasibility for the pivotal algal pre-treatment component of the proposal. Finally, as already touched upon earlier, feasibility is also tied to the extent to which the "Se treatment" component of the system can provide benefits without also providing substantive ecotoxicity. This proposal's focus on ecotoxicity assessment via chemical speciation of Se in biological samples is misplaced. First, existing literature does not indicate that there is high site-specific variability in the speciation of selenium in food web items such as aquatic invertebrates. Results of such investigations almost uniformly find about 60-85% organic selenium, and there is no theoretical reason to suspect that the proposed wetlands would produce any different results. Secondly, without doing feeding trials, there is no basis for interpreting fine grain variation within the above cited range with regard to "bioavailability". Presently, it cannot be stated with any empirically measured certainty what the difference in risk is, if any, for example, between 10 ppm brine flies with 60% organic selenium and 10 ppm brine flies with 85% organic selenium. Thus, this proposal intends to collect biological risk data that are only speculatively interpretable at best. The proposed invertebrate sampling fails to include collection of sediment cores for benthic larvae which have been found to be the highest risk invertebrate taxa at San Joaquin Valley (SJV) evaporation ponds. By the time these taxa might be sampled in emergence traps as emerging adults they have already shed the larval exoskeletons that have been documented to contain up to 300 ppm Se at SJV evaporation ponds. The proposal to collect bird excreta samples is undeveloped and suggests a critical lack of expertise in this area. It is not revealed how the researchers will know what species of bird such samples are from, whether the samples have come from birds that have actually been feeding at the system (as opposed to just loafing birds), or how samples that are uncompromised will be obtained (one can't just scrape them up off of soil, much of the excreta is liquid and is immediately absorbed by soil). To pull-off the proposed bird excreta sampling would require detailed observation of birds to confirm a sustained feeding event at the wetland, then the capture of such bird(s), and then the holding of such bird(s) in captivity in an enclosure specifically designed for collection of uncompromised excreta samples. No one on the proposed research team has the expertise to do this kind of work (nor apparently to even plan such work). Finally, the

results of such work would be impossible to interpret. For example, if the excreta samples are low in organic selenium how would one know if that was because the bird's diet was low in organic selenium or because organic selenium was differentially retained in the bird's tissues compared to excreta? The proposal would be much stronger if the proposed ecotoxicity assessment focused first on documenting the utilization (or lack thereof) of the treatment test site by breeding water birds and the associated selenium content of avian eggs along with appropriate measures of avian reproductive performance. There were other questions about the feasibility of this proposal, but the above comments are sufficient to reflect the score for this criteria.

REFERENCES

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**FUNDING RECOMMENDATION: $0**

Per the Financial Assistance Program PSP 2012, if a "0" score is received for any of the four evaluation criteria, the applicant, and therefore the proposal, will be disqualified. This proposal received one "0" score in the Consensus Review.